[From the Am. Journal of Science and Arts, Vol. X, Supplement No., 1875.]

THE SOLAR ATMOSPHERE,

AN INTRODUCTION TO AN ACCOUNT OF RESEARCHES MADE AT THE ALLEGHENY OBSERVATORY.

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Paper read at the Detroit meeting of the American Association for the Advancement of Science.

It has long been observed that the sun is not everywhere equally bright, but that the edge is darker than the center, and since a self-luminous sphere should appear of sensibly uniform brilliancy, so as to present to the eye the appearance of a flat disc, this diminution of light must be due to some medium external to the photosphere. Accordingly, even before the invention of the spectroscope, it was admitted that an absorbing atmosphere surrounded the sun and that the effect would necessarily be to diminish the amount of radiation. La Place, in the tenth book of the *Mecanique Celeste*, has attempted to compute the total effect of this absorption, from data furnished by the observations of Bouguer, and considers that were the solar atmosphere removed, the heat and light would be twelve times as great as at present. Though this value is erroneous, being deduced from imperfect data by processes which rest on a false hypothesis, it may yet serve to call attention to the importance of a somewhat neglected field of research, in which this observatory has been in part occupied during the past three years.

Several estimates of the absorptive power of this atmosphere have been made, differing widely from each other. According to that of La Place just cited the absorption is about $\frac{11}{12}$ of the sun's emission, according to Liais less than $\frac{1}{12}$; according to Secchi ·88. These discrepant results may be due in part to different hypotheses used in computation, but they are in every

case founded on an experimental comparison of the light or heat of the sun observed at the center, with that observed near the edge.

This direct comparison of the two lights involves, of course, no hypothesis, but is simply a photometric measurement, and apparently an easy one. Let us examine the result of this comparison at different hands. Arago* using the Rochon prism and analyzer announced as the conclusion of a prolonged research, that the light at the center of the solar disc must be diminished by $\frac{1}{41}$ part, or 2·4 per cent to equal that of the edge. Liais,† announces from comparisons carried on by the extinction of one light by a stronger, that the central light should be diminished by about 10 per cent to equal that of the edge. His result is then four times as great as Arago's. Secchi, with a wheel photometer, finds for a nearly corresponding point ·78 per cent as the amount by which the central light should be diminished. This result is then *seven* times that of Liais, and *thirty* times that of Arago.

Secchi has made observations on radiant heat to be found in *Le Soleil*‡ the most important results of which are that the heat diminishes from the center to the edge; that for a given point there is a satisfactory agreement between the absorption of light and heat; and that the equatorial regions are hotter than the polar. The present writer has made somewhat extended researches in the same direction which are chiefly unpublished. Vogel has made interesting researches in the relative actinic absorption.

None of those who have attempted to compute the amount of the solar absorption appear to have drawn conclusions from their results as to its effect on terrestrial temperatures, and yet if the absorption be anything like what has been found by La Place, and by Secchi, using La Place's formulæ, the subject deserves attention.

It will be shown in a forthcoming memoir that the absorption is much less than these values, but that definite limits can be assigned, within which it must lie, and that at any rate it is of such importance that the conditions of animal life upon this plenet largely depend upon it. It will further appear probable that with a slight change in the depth and absorptive power of this atmosphere, fluctuations in terrestrial temperature will ensue, very great in comparison with any actually observed within historic periods, and it will be shown that this atmosphere is not in a strictly stable condition. Though the subject

* Oeuvres Completes, Tom. i, p. 235.
† Memoires de l'Academie de Cherbourg.
‡ Le Soleil, 2me edition, Paris. 1875.

then is a nearly neglected one, and the methods of investigation of less apparent interest than those of the spectroscopist, the results are, if verified, of a peculiar interest, from their bearing upon the possible changes in the mean temperature and climatic conditions of our own planet.

The portion of this atmosphere chiefly concerned in absorption I have been led to believe from several considerations is extremely thin, and I am inclined to think it is nearly identical with the "reversing layer" at the base of the chromosphere observed by Secchi and Young, though the other chromospheric strata doubtless, have some share in the obscuration made, and, in a still less degree, the other solar envelopes.

The methods used for heat measurements at Allegheny have been partially described,* and need not be enlarged upon here. Those for light are believed to be novel in their present application, and may be given briefly.

We have seen that a photometric comparison of the center and edge of the sun is for some reason attended with risk of error which we should not anticipate, but which must be great from the enormous discrepancies existing among practiced observers; for, taking Arago's value as the standard, Liais' is four hundred per cent, and Secchi's three thousand per cent greater. Now skilled observers, in comparing their estimates of the excess of the light of a gas flame over that of a candle, for instance, by the familiar methods of ordinary photometry, need not be expected, as common experience shows, to commit errors which are in any way comparable to these.

If such familiar means as the Rumford and Bunsen photometers could be used for the *direct* comparison of the solar intensities there seems no reason why the results should not be of the exactness obtainable in the physical laboratory, or at least of the same order of accuracy. If results can be obtained by such means, they may at least be hoped to be free from gross error.

At first sight it appears that these methods are not applicable to the direct comparison, for the ordinary use of either photometer supposes that the relative distances of the lights from the screen can be altered, while we cannot change the relative distances of the lights to be here compared, which are two portions of the sun; and, if we attempt to enfeeble the stronger lights by shades, or by diffusing it through a lens till it equals that of the other seen direct, we lose the peculiar advantages of the methods which assume that the lights are viewed under the same conditions and measured by the relative distances from the screen.

I arranged, in June, 1874, an apparatus which appears to be free from these objections, and which is here described in

* Comptes Rendus, May 22, 1875.

principle, not in detail. Let us suppose that to the equatorial is attached a screen upon which is described a large circle whose radius is divided into 100 equal parts, and whose center is always in the prolongation of the optical axis. This screen receives an enlarged solar image from an amplifying lens. To fix our ideas we may suppose that the circle and image are each always 24 inches in diameter, and that a direct comparison is to be made of the light of the center with that of a point $\frac{3}{4}$ of the way from the center to the western edge of the sun. Two small* and equal circular apertures are made in the screen at points equidistant from the center or 75 divisions of the scale apart. The telescope is directed to neither point under examination, but to a point of the solar disc $\frac{3}{8}$ of the way to the western edge from the center, and which is therefore midway between the points to be measured. The image of the center now falls on one of the small apertures in the screen; that of the point under examination on the other, and through the apertures pass two cones of rays each slightly divergent, formed under precisely the same conditions, since they come from the same lens and are equidistant from the optical axis. Two prisms of total reflection (cut from the same piece of glass) are placed one behind each aperture, and these deflect the rays toward each other, so that the reflected portion of the cones have a common axis behind and parallel to the screen, within a chamber which is lined with black and shielded from light, so as to form in fact a camera obscura. There is a small Bunsen disc behind the screen and in the camera, whose center remains in the axis common to the reflected cones as it (the disc) slides back and forth on a graduated scale which measures the distance from the source of illumination. Evidently one light is diffused and the other concentrated by the disc's advance and recession upon the cones, sensibly as though these were formed by lights at their virtual apices beneath the screen, and by lights which were of the color and intensity of the portions of the sun under examination. In practice it is found convenient to introduce a lens of short focus between each aperture and its reflector, so that the cones, while still exactly similar, may be less acute. By the substitution for the Bunsen disc, of a small box, sliding also on the graduated scale, and containing a rod whose shadows are cast, by means of a second pair of reflecting prisms, on a prepared surface, sliding with the box, we convert the instrument into a Rumford Photometer, and in this form also it has been much used here. This application incidentally permits under very favorable circumstances a comparison of the *color* of the light from different parts of the sun, which is by no means uniform. When light

* The diameter of the aperture is on the above scale about 0′·10″.

from near the edge is juxtaposed with that from the center, the shadow illuminated by the former is chocolate-red,* while that from the latter is a peculiar bluish tint, nearly like that given by the shade-glass called by English opticians "London Smoke." (I use these comparisons not being able readily to assimilate these colors to any of the pure spectrum.) It is worth while to remark that this observation confirms others obtained from different means by the writer who has elsewhere announced the fact of a selective absorption in the solar atmosphere of such a nature that were the envelope removed the gain in light would be greater than the gain in heat. The light we receive from the sun and universally call "white" is thus seen to be, upon the whole, less refrangible than that which the sun would emit if deprived of its atmosphere; in which case it is evident that the color of our luminary as it grew brighter would tend toward *blue.* If then the depth of its atmosphere were sufficiently increased, our sun, in growing darker, would also appear more *red.* To more than suggest the possible influence of such a cause on the colored stars, among which the bluish are distinguished by the absence of absorption spectra, and the desirability of ascertaining whether change in color, if any such are verifiable, are or are not accompanied by slight changes in apparent magnitude, would lead us away from the present subject.

So clear an exhibition of color in the actual comparison is a sign of the delicacy of the Rumford method, but it introduces a disturbance in our estimation of the intensities of colored lights. An arrangement of the Masson photometer (in which black and white sectors on a rapidly revolving disc illuminated by the colored lights are viewed by the intermittent electric discharge) has been prepared to obviate this; but the complete examination is intended to include that of juxtaposed spectra formed from the lights under examination, selected portions of which of different wave lengths will be compared, as regards intensity, by processes which are a development of those mentioned above. In all such comparisons the apparatus is reversible, so as to eliminate any inequalities in the material, position, or condition of the rays under examination due to the instrument; and in practice corrections for every form of instrumental error are applied which are not here mentioned.

The methods above indicated are, with some slight modifications, evidently adapted to the comparison of the light of the sun-spots, and the light adjacent to the limb, with that of the center; to studying the rate of diminution of light without the disc, and to like investigations; and they are now being so used.

* This color appears to have passed unnoticed by all observers but Secchi, who remarks that the edge of the disc is smoky-red.

Without entering into detail, I may observe that the measurements of the light of spots, though as yet incomplete, warrant me in stating that the absorption of the more refrangible rays, though great in reference to the less refrangible, is not so great as would appear from published measurements. It has long been supposed that the umbræ of spots were not absolutely dark, while it has been admitted, from Dawes's observation, that there is within the umbra a sensibly black "nucleus," and from the assumed blackness of this "nucleus," several astronomers have been led to suppose that the "nuclei" are not openings into a dark gaseous interior.

The measurement of umbræ, and of so-called "nuclei" here shows not only that neither are absolutely dark, but that the absolute light of either is enormous, that of the average "nucleus," so-called, being, as I find, at least *five thousand times* that of the full moon.

La Place, in assuming that the radiation in any direction is constant, and is proportional to the radiating area (or that the radiation is infinite at the edge of the disc), concludes that the total absorption is represented by the integral

$$2\pi\int_0^1 \varepsilon^{-\frac{1}{\cos\theta}} \sin\theta d\theta$$

where θ is the heliocentric angle between the earth and the point of the solar surface under examination; and this assumption has been adopted by Father Secchi, who, in his latest edition of Le Soleil, gives results derived from the use of this integral. I shall, however, assume, in accordance with what seem to be the teachings of modern physics, that a globe as large as the sun whose photosphere, though composed perhaps of very light vaporous material, is yet opake at a limited depth, (as observations on superposition of cloud-strata have shown) that in such a globe radiation would be proportional to the cosine of the angle between the normal to the surface at any point and a line drawn from that point to the observer's position; or, that the sun, deprived of its atmosphere, would appear as a flat disc; whence it will follow that, the total radiation of the sun without the atmosphere being unity, that which escapes through the atmosphere will be, if we adopt La Place's formula with this correction, $1 - 2\pi\int_0^1 \varepsilon^{-\frac{1}{\cos\theta}} \cos\theta\sin\theta d\theta$.

These expressions, if La Place's assumptions are otherwise correct, should give, for any comparison of the heat at a given point of the disc to that of the center, a certain value of the absorption which should be constant for any value of θ. In fact I find, however, that this is not the case. The discrepan-

cies in the summation I obtain for different values are not however casual and irregular, as they would be if due to errors of observation, but are systematic. This formula even as here modified, though analytically speaking correct, yet appears to rest on assumptions which are in disaccordance with facts ascertained since La Place wrote. These facts, as they seem to me, demanding a changed formula I expect to point out in a definite form in a subsequent memoir. It seems fitter then to defer an exact statement of the limits of the amount by which solar radiation is absorbed by the atmosphere, until it is accompanied by demonstration. I will only here repeat that we may feel certain that the estimates cited from La Place and Secchi, and which make the sun's atmosphere absorb from $\frac{7}{8}$ to $\frac{11}{12}$ of the radiation we should receive in its absence, are in excess of the truth. For the sake of indicating approximately the real value, I may also state that from my computation of the so-called "luminous-heat" rays, not greatly less or more than one-half the whole are absorbed by, turned back by, or converted into work in, the sun's atmosphere. The total thermal absorption is somewhat less than that of the luminous heat. If, however, we admit this value provisionally, certain results seem to follow which deserve mention.

The mean surface temperature of our globe is separated from that of absolute zero by about 500° of the Fahrenheit scale. The internal heat supplied to the surface may be neglected. The temperature of interplanetary space is, according to Fourier -60°C., according to Pouillet -142, according to Liais -97. Liais, in a memoir whence we cite these estimates, admits, as a consequence of his own, that the obscure heat received at the upper ~~limb~~ of our atmosphere from space (that is, from the limit chiefly non-luminous matter which occupies it) is greater than that furnished directly from the sun, a conclusion which we are not called upon here to adopt, further than to observe that with this, as with either of the other estimates, the obscure heat received from interplanetary matter by reflection from the sun at the surface of our atmosphere is considerable, so that, if our luminary were wholly extinguished, the temperature of the earth would fall much below that it would reach if only the direct solar heat were withdrawn.

We shall perhaps be warranted in entertaining it then as a reasonable assumption, that, in the complete absence of the sun, the earth's temperature would fall very nearly to -273° C. We prefer as the basis of an estimate, confessedly but approximate, to take the mean of this value and Pouillet's, and, using the Fahrenheit scale, we may state that of the 500° F., which on the natural scale is the approximate mean temperature of our globe, as much as *four-fifths* is derived from the sun.

If it be true then that the sun is surrounded by an atmosphere whose principal action in obscuring the heat radiation is due to a thin stratum which cuts off one-half of the heat which should reach us, and in whose absence this radiation should be doubled,—an atmosphere not independent of the interior of its globe in such a degree as our own, but one to and from which matter is constantly being added and withdrawn, it follows that any change in the ratio of supply and withdrawal, or other cause, which should increase its absorption by so much as 25 per cent would diminish the mean surface temperature of our globe by 100° F., whilst a like diminution in the envelope would produce a corresponding change in the opposite direction.

I am unable to see that, if the Allegheny observations are correct, whence I have derived the above given approximate value of the absorption, any other result would follow, and in any case the existence of life in its present forms on this planet seems dependent, within certain limits, on the depth and absorptive power of the solar atmosphere. The reader who may not have closely observed how far the validity of this result is dependent on, and how far independent of, the hypothesis just made as to the mean terrestrial temperature, is invited to remark that there is nothing hypothetical in any case in the assertion that the earth would fall, in the absence of the sun, to a temperature as low as any actually observed on its surface. If any value attach to the writer's measurements, then, it will be found from a repetition of the calculations with (arctic) temperatures *actually observed*, and hence within the truth, that the result remains that a comparatively slight thickening of the solar atmosphere, or an increment of its absorptive power by as little as $\frac{1}{4}$, would bring the planet back to what we must suppose were the temperatures of the glacial epochs.

Father Secchi seems to think it probable that the difference between the measurements of the heat at different portions of the solar disc made at Rome in 1852 and at Allegheny in 1873–4 are explicable by a real change in the atmosphere of our luminary. To the writer it seems that such a change as Father Secchi assumes, if real, is (if interpreted by Father Secchi's own formulæ) likely to have already altered the mean annual temperature of the earth to an amount sensible to every inhabitant of its surface. He is himself, therefore, of the opinion expressed by M. Faye that no evidence of real variation has yet been established.

It will not of course follow that changes may not take place in cycles of time long with reference to historical periods. All analogy leads us indeed to think an absolute uniformity most improbable.

Finally then, it appears that though we are justified on grounds established by Helmholtz and Ericsson in believing in the constancy of the heat supply *within* the solar envelope for periods to come, which are with reference to our terrestrial past, almost infinite, we are not justified by our present investigations in asserting that the solar radiation has been constant during geologic periods, or is certain or even likely to remain what we now see it during corresponding periods in the future.

If there be great cyclical changes of long period (and in our present ignorance we can only say that such are not antecedently improbable) there will be corresponding changes in terrestrial temperature, and it is allowable to inquire whether we do not find here matter for consideration in connection with those great changes of temperature in past epochs which have in them nothing hypothetical, which geology assures us have indeed existed, and for whose possible cause no satisfactory suggestion has hitherto been made.

ERRATA.—Page 494, for "neucleus" read "nucleus," and for "neuclei" read "nuclei."